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SPACE AND TIME

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AN EXPERIMENTAL PHYSICIST'S CONCEPTION OF THESE IDEAS AND OF THEIR ALTERATION

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PREFACE

"Celui qui comprend ne hait pas."—AMIEL

THE following pages, with the exception of a few minor additions and alterations, comprise the substance of a lecture recently given by the author to the Students' Club of the University of Stockholm.

The enormous mass of literature which has grown about the present theory of relativity contains dispassionate accounts of the theory and its mathematical development, exuberant tributes of approval, and, in some cases, simply malicious diatribes.

The aim of this lecture was to deal with a subject to some extent neglected

—namely, the elucidation of the foundations, the starting-points, of the new theories, whether admirable or offensive in themselves. The purpose was not to understand them, but to understand to what extent they may be necessary or otherwise from the standpoint of science in general.

C. B.

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INTRODUCTION

STIMULATED by a communication from Professor Soddy concerning Professor Benedicks and his book, and knowing the public spirit with which Messrs. Methuen have issued a number of books round about the subject of Relativity, I acquiesced in a suggestion that a translation of Professor Benedicks' writings, as representing ideas of general interest, might be published, whether one were able to agree with them or not.

Professor Benedicks is Director of the Metallographic Institute of Stockholm, and looks at physical subjects from a new angle. His view of Space and its measurement is based on the idea of an immutable solid,

which he uses as the instrument of a curious kind of concrete geometry; and he connects this ancient idea of fixity and unalterable permanence with the name of Thales. His view of Time, on the other hand, is associated with the equally venerable name of Heraclitus, and his universal flux. In any region where there is no change, the author would say that time stood still; whereas I should prefer to say that although all processes may cease, nothing physical can alter the inexorable march of time. So also I should not in the least rebel (as the author does) against a FitzGerald-Lorentz Universal Contraction as upsetting to geometry; because I hold that geometrical propositions, being abstract, cannot be affected by the misbehaviour of an otherwise immutable solid. Others, however, may find the author's position attractive. Again, I find no difficulty in experiencing

an absolute rotation ; but the author does, unless he is allowed to fire off some projectile of infinitesimal mass which by obeying the first law of motion can serve as a zero of reckoning.

The author tries to get over the difficulty of ascertaining synchronism, by postulating an absolutely rigid mode of connexion between two distant stations, by aid of his immutable solid ; and he seems to make the velocity of light dependent on his definition of time. This last seems in one sense to be an extension of the tendency, begun by Relativity, to treat only of what can be sensed and measured, rather than deal with impracticable abstractions ; but although the author substitutes imaginary concretions for our usual abstractions, he rejects all the main relativity notions, as leading to results too hopelessly complex ; he especially finds fault with the strange

law for the composition of velocities founded on the Larmor-Lorentz transformation.

One main outcome of the author's philosophic view appears when he vigorously sustains an emission theory of light against the undulatory theory. It is well known that a modified emission theory, or rather a combination of emission and wave theory such as Newton himself really began, is more and more attracting the attention of physicists. A wave theory needs an ether, and even Newton's modified emission theory needed an ether too; but a full-blown emission theory, explaining everything by bombardment,—in fact, a universal projectile theory,—needs no medium at all; the flying particles are sufficient for universal contact action. Difficulties about contact, and what is meant by collision, are rampant; but if these can be unattended to, the apparent simplicity of an emission theory is

superficially attractive. Moreover, it has the advantage of explaining away the Michelson-Morley experiment without any difficulty at all. It seeks to get over the Foucault difficulty, about less speed inside dense matter, by discarding the Newtonian postulate of corpuscular attraction and endeavouring to apply Huyghens's Principle to refraction, even of corpuscles.

That emission of luminiferous particles is inconsistent with relativity doctrines is obvious, inasmuch as the velocity of light must then vary with the motion of the source: and in many other ways it is in opposition to the Einstein view. The real and ultimate theory of light, when forthcoming, seems to me likely to be a combination or composite affair, not so simple as either the emission theory or the wave theory pure and simple. Meanwhile the author argues ingeniously, and shows that

he is widely read in the speculations of his time. He quotes the brilliant Swiss physicist Ritz, who died at the age of thirty-one, in favour of regarding light as an emission into empty space, and in favour of abandoning a stationary ether and the whole Maxwell-Lorentz electromagnetic theory, now that in the light of Relativity they involve so many curious complications.

If physical science were not in such a state of flux at the present time, this book might be treated more cavalierly; but as I presume no one is quite sure in which direction the ultimate line of progress points, it is not safe to turn down any contribution of a thoughtful and reasonable physicist who is acquainted with the facts.

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INTRODUCTORY

EXACT science of the present day exhibits two striking characteristics. On the one hand, empirical results, of deep significance, have given to man an insight into the microcosm of the atom, such as the boldest imagination would hardly have conceived possible a few decades ago; on the other hand, a purely theoretical effort to discern what might be called the sign of the macrocosm has striven to reach by means of mathematical reflection a new and more comprehensive theory of physical reality.

Concerning empirical results, no one will dispute that experimental research has yielded results of permanent value and incomparable fruitfulness; further, that by these results is justified the bold, yet

classically simple philosophy developed by Greek thought. The conception of matter as composed of atoms in space,—the conception of Democritus, — has after two thousand years gained a definite victory.

Regarding the theory, however, opinions are far more divergent. Partisans of the one side consider that the speculations of the last few decades, which have culminated in the theories of relativity, have presented us with a logical structure comparable with and even surpassing that of Democritus in boldness, and that of Copernicus and Newton in mathematical beauty and power. On the other side there are opponents who proclaim that the speculations in question, belonging to the beginning of the 20th century, are akin to the now discarded philosophy which was highly appreciated, especially in Germany, at the beginning of the 19th century. They hold that its undoubtedly abstruse and complicated nature will render impossible its general acceptance; that the fundamental

conceptions, such as those of time and space, are herein used not only in a new sense, but in a sense that varies according to circumstance; and so forth. In short, opinions have clashed with uncommon violence, so that there has been of late a certain tendency to deny to persons who are not fully trained in mathematical physics their right to criticize the new theories. I have felt it my duty to pronounce a decided No against such a restriction.

It is not my purpose to give an account of the contents of the theories of relativity —which have been many times, as the saying is, "gemeinverständlich dargestellt," or put in an "easily comprehensible" way. My purpose is hardly even to criticize the theories themselves, but rather to examine the starting-points. Here we may all agree that the new efforts in thinking possess the one great merit, or perhaps, as we may choose to regard them, the great fault, of having forced present-day

scientists—partly to the detriment of what they might otherwise have done—to attempt to gain clearness of conception concerning questions which might otherwise have left them indifferent.

For my part, it has seemed essential to try to come to a clear understanding of the inner meaning of the fundamental conceptions of space and time. If the most subtle minds have for some thousands of years evolved conceptions which are now to be broken up summarily—perhaps in order to fuse them into some higher and more pregnant synthesis—then the first demand of a scientific mind will be an elucidation of the fundamental character of the old conceptions. In modern metallography there is a sound principle which leads us to investigate simple metals before investigating their alloys. A similar precept is commendable with regard to the conceptions of space and time. Why should space be considered to possess precisely three dimen-

sions? Why should time be thought of as flowing in one direction? It is surely a straightforward scientific desire first to gain clarity with regard to these questions, before proceeding to change the fundamental conceptions. I have sought in vain in the literature of the subject for satisfactory expositions.

In relativistic accounts, we find attempts to characterize the conceptions of space and time as in themselves defective, and therefore in need of something to take their place. An example is contained in the following quotation:—

"Even 'Space' and 'Time' are only provisional and auxiliary ideas, belonging to an untenable conception of the universe. In reality 'Space' and 'Time' have *no existence* in an absolute sense. Even Space and Time depend on the point of view taken." [1]

[1] K. Witte, *Raum und Zeit im Lichte der neuern Physik*, Sammlung Vieweg, vol. 17, p. 8, Braunschweig,

Perhaps the results of the efforts of an experimental physicist to gain clearness on these points may be of interest to others. I should like to remark beforehand that here the exposition is not worked out in detail, but has merely the character of a sketch.

1914. "Auch 'der Raum' und 'die Zeit' sind nur provisorische, der Wirklichkeit nicht entsprechende Hilfsbegriffe eines unhaltbaren Weltbildes. In Wirklichkeit existieren 'der Raum' und 'die Zeit' im absoluten Sinne nicht. Auch Raum und Zeit sind Standpunktsachen."

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VICTOR RYDBERG, of the University of Stockholm, the great contemplative poet of the North, speaks about

> ". . . time and space,
> A terrible and boundless prison-house,"

from which we human beings are never able to free ourselves. And no doubt he is right. What we may be able to escape, however, is the obscurity, the mystery which cling to these conceptions. As a matter of fact, space and time are conceptions which have come into physics from the common-sense sphere of the everyday man; and we thus have reason to expect that they may be expounded in a manner clear and comprehensible. Let us start with the very simplest; namely, the measurement of length.

LENGTH

SURROUNDED by the varied multiplicity of Nature, man at a very early stage arrived at the conception of *immutability*. This conception would hardly have arisen, however, had we not been living in an environment which presented innumerable and striking instances of those phenomena which we call *solid bodies*.

To make clear what we mean by a solid body is simple enough, provided we may make use of geometrical definitions. These, however, are derived from the body, and to use such a procedure in reaching our definition would mean arguing in a circle. Perhaps the only statement we may be able to use as a logical definition, is the assertion that *a solid body is an object characterized by immutability* under all ordinary circumstances, such as transference from

one place to another. An ideal solid body is thus by definition an immutable object. This immutability must not, of course, be considered as excluding the fact that by an external action an actual solid may change its form or shape.

This "principle of the (absolutely) solid body" seems to play essentially the same rôle in exact scientific thinking as the "principle of identity" plays in thinking in general. The latter principle is known to imply that under all circumstances "every conception (or every object of thought) is identical with itself"—that is to say, it is immutable. According to logic, human thinking in general is based upon the *principle of identity*; similarly, quantitative natural philosophy, in the opinion of the present writer, is based upon the *principle of the solid body.*

It might be asked, how far the latter principle is to be regarded as in itself a necessity of thought. Actually, it is

not of necessity connected with thought. An imaginary organism endowed with the power of thought and living always amidst the water-drops within a cloud would never arrive at the idea of the solid body. For an *Amoeba proteus* floating in water, and provided with the faculty of thinking, would be far from conceiving a solid body. Yet for human beings the conception comes easily, as being applicable to objects constantly encountered. A piece of marble, of granite, or of diamond realizes very nearly *the idea of the solid body.*

Assuming that we are in possession of this Philosopher's Stone—as we might call it with a certain degree of propriety—we easily arrive at the fundamental conceptions of geometry. We first think of a solid body being shaped in the manner actually adopted by precision workers to produce a flat surface; that is, a first piece, A, is given such a form that its surface coincides exactly with the surface

of another piece, B. This, as experience shows, may be accomplished, by means of suitable grinding materials, to any desired degree of accuracy. A third piece, C, is then shaped the same way, so that its surface will likewise coincide with that of A. If now the surface of C also coincides with that of B, then by definition the bounding surface—identically the same for A, B, C—is called a *plane surface*, or a *plane*. In case B and C do not coincide, we have only to repeat the procedure until at last the coincidence is obtained as accurately as desired.

If two plane surfaces thus defined are made to cut through one and the same solid body (which experience has shown to be quite possible) in such a way that they intersect one another, the line of intersection, or the common boundary, is called a *straight line*.[1]

[1] Compare the accurately straight rule used in connection with the standard measuring set of Mr.

A *point* is defined by a third plane, intersecting the said line. Consequently, a point is the intersection, or the common element, of three planes.

Two points on a straight line determine *a length.* A length is said to measure the *distance* between two points. Thus the distance between two points of a solid body is, according to the fundamental assumption, something immutable under all circumstances.

Further development leads to other conceptions of Euclidean geometry, such as parallelism, geometrical figures, and so forth.

As *unit of length* we define the distance between two points upon a solid body which has been arbitrarily accepted as a norm. Thus we assume that the standard unit of length in Paris, consisting of a rod of

C. E. Johansson, and used by the National Physical Laboratory.

platinum-iridium, to be characterized, when embedded in melting ice, by such an immutability that it realizes sufficiently well the conception of a solid body. Certainty in this respect can never be obtained, although the probability is strengthened by repeated comparisons with quasi-solid bodies, or with waves of light. But there is nothing to prove that all the lengths of our solar system, and wave-lengths likewise, do not alter continually, either periodically or quite irregularly. So far as we can see, this lies entirely beyond the possible knowledge of man. In any case, the main point is that without the assumption of the absolutely solid body, no exact geometry is possible.

By the aid of the unit of length, and in a manner which needs no description, we produce upon a solid body a *scale of length.* The divisions of this scale, from the assumed immutability of the unit, even if it is moved, are of identical length. Hence we obtain

measurement of length, co-ordinate axes, and so forth.

It is quite possible to apply any number of co-ordinate axes (or planes) to a solid, but only *three* are *necessary* in order to determine the position of any point.[1] If more than three axes are used in the co-ordinate system some additional stipulations must be made in order to avoid confusion. This is the ground for saying that a body has *three dimensions*.

What has been said about the geometry of the solid body is extended by a natural and easy process of abstraction to the

[1] It would be quite practicable to define a "point," or rather "standard point," as being the intersection of four planes; in other words, when the distance between two points is zero, they form a "standard point." This would signify that we may accept a point as a definite "standard point," in representing an empirical result, only if two different methods of observation have given coincident results. In such cases one would always have to consider four co-ordinates of dimension; this would, of course, be more complicated, but certainly not impossible.

conception we call *space.* We make, so to speak, the solid body *sublimate* or disappear, leaving behind only its geometrical definitions.

Like the solid body, space is commonly said to have three dimensions. As a matter of fact, we may attribute to space as many dimensions, or co-ordinate planes, as we like. In that case some additional stipulations would be required, which, though not included in the fundamental conceptions of geometry, might still be sharply defined.

A form for such additional stipulations, which is often used in practice and which enables us to apply to space a greater number of dimensions than three, is to indicate different dimensions *in different colours or in lines differently drawn.*[1]

[1] The most familiar way of applying more dimensions than the number generally considered available, occurs in every *perspective drawing.* Here a "two dimensional plane" is used to represent three dimensions. However, to avoid ambiguity, it is necessary to utilize some additional stipulation or convention; such, for instance, as

The assertion that space has only three dimensions, signifies therefore simply that we cannot make use of more than three, unless we also adopt some special means to avoid confusion.

Here we touch upon the question, is it a necessity for geometrical thinking to state that space must have three dimensions? As Poincaré pointed out, this is not necessary. The three dimensions imply the freedom of movement given to Man. A flat larva, whose universe consisted of the very thin layer between the wood and the bark of a tree, and who had the faculty of thinking geometrically, would not be able to reckon with more than *two* dimensions. A rod-shaped bacterium living in and filling the lumen of a capillary—such, for example, as the microscopic tubings of iron oxy-hydrate

to draw the nearest lines heavier than the distant ones, or to use shading according to a given direction of light, or to assign light absorption to space ("air-perspective"); or similar practices.

which exists in bog-ore—would probably not be able to take account of more than *one* dimension.

We have here considered no other geometry than that of Euclid. The statement frequently occurs that non-Euclidean geometries, that is, the innumerable possible geometries in which the parallel axiom of Euclid is invalid, possess a higher value as an expression of truth than that of Euclid. We wish to say here that the Euclidean geometry is decidedly the *simplest* one for our conditions. It is meaningless to ask which geometry may be the truest; the only question is which, under given circumstances, may be the most convenient one.

In dealing with space it is difficult to pass over the question whether its extension, which experience has taught us is very great, is also infinite. It seems to me obvious that it would be altogether outside our limitations to claim this to be the case.

If thinking inhabitants were existing in a microcosmos built up of the almost innumerable atoms, each comparable to a solar system, within a tiny air-bubble in a block of glass, surely they might easily arrive at the conclusion that their universe was boundless, inasmuch as its boundaries lay altogether beyond their experience. It would certainly be conceit on the part of us earth-worms to believe ourselves to possess power to decide whether space, cosmically taken, is limitless or not. Here it certainly seems justifiable to say "*ignorabimus.*"

TIME

THE search for the Permanent, the Unchangeable, the Eternal, in the manifold variety of Nature appeals to every thinking human being, and has always done so. The oldest of the Greek philosophers, Thales of Miletus, sought, in what he called the primary matter, " the unconditioned unity (unalterably fixed element) in all change and multiplicity." Thales may well be called the *Apostle of Immutability*. The insufficiency of his philosophy, however, soon brought forth a reaction. Heraclitus, " the obscure," claimed Change to be the most essential feature of existence. Everything in nature changes, everything is flowing. And no

doubt *Change, Happening, Process* are experiences quite as important as Immutability; Life as important as Death.

Is change a necessity prescribed by thought? By no means. Experimental scientists of the present day (K. Onnes, W. Nernst) have enriched our experience with the knowledge that at certain temperatures, which although very low, are nevertheless a good deal higher than was originally believed, every change in the position of atoms ceases.

Among the innumerable changes or processes which early attracted attention, men noticed those of a particularly simple kind; namely, those giving the impression of *regularity*, or in other words, *those which in spite of their changing nature produced an impression of immutability.*

A change of this kind is, for instance, that which occurs in the case of a spinning top. Consider a well-turned top, with its sharp point placed upon a smooth surface,

spinning with the direction of its axis steady and unaltering. If it did not carry a visible mark or dot, such that its varying position with relation to the fixed base may easily be observed, one would hardly comprehend, at least at first, that it represents something changeable.

In a way analogous to that in which the length of a certain "immutable" solid body was chosen as a comparative means of studying solid bodies, one may also and naturally introduce some variable process which gives an impression of immutability as a comparative means of dealing with the study of changes in general. The simplest way to obtain such quantitative measurements would be, in the case of the spinning top, to take the length of an arc—measured from a given radius or point on the fixed base—through which the observed point has travelled. This would provide a measure for what might perhaps have been called *length of process*, progressive course

happening, or the like, but which actually is called *time* (with reference to the process in question).[1]

As the *unit* of the variable thus introduced, it would suggest itself to us to choose the period which corresponds to two successive coincidences of the mark on the

[1] We have made no attempt to *define* the fundamental conceptions used, such as that of immutability, and we may be allowed to take up the same position as regards the conception of time. The following remarks, however, may be permitted.

Briefly, *time* is the new item which is added to the experience of Man when proceeding from the sense-perception of immutability to the more advanced perception of mutability. For instance, the length of a given object does not necessarily give an observer the perception of immutability; just as often it gives him the complex impression of having *different lengths*. Now time is the variable which we have arbitrarily introduced, in order to enable us to compile such different impressions in a systematic way. The first qualitative idea of time is met with in the word *successively*, or its equivalent. We say the different impressions given by an event have taken place *successively*. The quantitative idea of time demands, as stated further on, the introduction of an auxiliary process, presumed to preserve the same character throughout.

top with a given point of the plane. The *unit of time* thus defined, namely, one complete revolution of the top, is to be considered as immutable with the same degree of correctness or incorrectness as the whole process of rotation of the top itself was regarded as immutable.

It is, of course, only on rare occasions that in the laboratory we make use of a top or cylindrical wheel, which we ourselves have set into rotation, as a time-measuring device. In the earth, the sun, the moon, and the vast multitude of heavenly bodies, "we have tops of gigantic dimensions." Naturally, the earth's rotation, on various grounds of probability, appears to be vastly superior to any artificial top or gyroscope as regards immutability of motion ; and for that reason has always been accepted as a standard for time-measurements. The astronomical day is, of course, a period of time corresponding to one complete revolution of the earth taken in relation to an

earth radius which is drawn to any fixed star.

Before continuing, an important point has yet to be considered. Let us return to the top, which seems to offer a somewhat clearer example of an original time-giver. The above definition of time refers primarily to the arbitrarily accepted rotation process itself, and the question arises, how its time-indication may be applied to another process which might occur at a great distance. The simplest example is that where the second process is also an identical, rotary one—that is, another identical top, rotating on the same fixed plane. Simultaneity or synchronism is said to prevail if a radius vector of the one is always parallel to a corresponding radius of the other. We ask, in what way can this definition be applied? Evidently, it can be realized in the way used to synchronize two paired wheels of a locomotive; that is, a solid movable connecting rod is pivoted at the end points of

two radii, where the length of the rod is equal to the distance separating the two axles. As the two radii have been assumed to be equal, they will during the motion also remain parallel. In principle this will fully define simultaneity, so long as the axes of rotation remain parallel. The first rotating body, A, is the standard which determines time ; the second body, B, may act as a *clock* or timepiece, by exactly reproducing A's time.

The clock plays the same rôle in the measurement of time as the pair of compasses in measuring length. Just as we assume, *a priori*, that a pair of compasses may be turned and moved in any direction without altering their length—though we actually know nothing about this—we also assume, *a priori*, that a clock may be turned and moved on the spot without its indications of time being affected. (It must be decided in each individual case to what extent a rotation around the axis will

require the introduction of a correcting factor.)

Accordingly, it will be possible to simplify the definition of synchronism for distant places. We say that *two distant clocks are synchronous, provided that their hands are moving as though their axles were connected by one rigid axle, consisting of an absolutely solid body.* This is the simpler form of synchronizing frequently used,—for example, in synchronizing two wagon-wheels belonging to the same axle.[1]

This definition of synchronism is precise, and has no ambiguity. It is founded only upon the fundamental basis for all measurement of time—the accepted unchangeability of the rotation process chosen as standard—and upon pure geometry—the fundamental basis of which is the existence of the absolutely solid body.[2] We remark

[1] Rate of torsion propagation along the axle must be infinite.

[2] In principle the use of a rigid axle in the definition of simultaneity is precisely as admissible as the use of a rigid rule for the definition of equal lengths.

that this definition is, as it should be, completely independent of whatever ideas we may have concerning such physical phenomena as the velocity of light—a quantity evidently dependent on the definition of time. It is independent of a supposed motion of the observer, and so forth.

We may further consider a few questions of a more philosophic nature. Is a reversal of time strictly impossible? If this is not the case, why does time always progress in one direction only?

Let us consider the two tops. The one, A, is selected as the time-standard; the other, B, has a period of revolution to be measured. Both are assumed to be rotating clockwise. For B, time is defined by the arc-length, reckoned clockwise from a fixed zero-point, which is traversed by the mark on A. Is it possible for the time so defined to be reversed? Certainly, from a formal point of view; and this in two ways. The first would entail altering the direction of

rotation of A; but this is excluded in our case *a priori*, inasmuch as A's motion was assumed to be unchangeable. Only the licence of poetry allows us to say

> "The hands of Time have travelled backwards,
> Here Medieval Time prevails."[1]

But, on the other hand, the direction of B's rotation may be reversed, which is not excluded *a priori*. The relation between the position of a mark on B and the arc-length of A would then change sign, as compared with the previous relation, and we might be justified in asserting that time now is going backwards in this process.

As is familiar, we receive this impression if the direction of motion of a cinema film is reversed. *Full reversability* of the process in question, in a physical sense, must be regarded as a necessary condition. But if the example of the reversed cinema film

[1] The Swedish poet, C. D. af Wirsén, "To the old city, Gosslar."

gives the impression of something artificial, let us take another case from Nature. The cells of a living plant are absorbing nutrition. Cell-division takes place; the plant increases in size; after one month it is *so* high, after two months *so* much higher, and so on. We now ask, is it conceivable that this growth or ageing process can be reversed? The answer will be that if the period of observation is short enough, such a reversal is possible. The swelling of a single cell may certainly be thought of as reaching a limit, and the cell then diminishing—that is, recurring to a *younger* state; but this holds true for the plant only if the period of time has been so short that a new cell has not yet formed. The formation of new cells is, as we know, an *irreversible* process. It cannot be made to go backwards. For a system of reversible processes only, time may perhaps be considered to go backwards; but in Nature irreversible processes predominate, and the " hands of

the clock" cannot be put back. The continual flowing of time in one direction and the second law of thermodynamics are only different aspects of one and the same fact.

Another question is whether the conception of time has any general validity, or if we are merely able to apply it to systems in which we are able to carry out measurements of time (Einstein). To my mind, the answer is simply this: the conception of time has signification only for systems *in which changes occur*. The conception of time may be said to be void of meaning in a world without changes. Such a world is *timeless*; at least, we may say its time is standing still. Time could be said to have been at rest for the mammoths frozen in the ice of Siberia. But, on the other hand, it is obvious that the conception of time, equally with that of length, can, if desired, be applied in cases where we have no opportunity of determining it physically.

MASS

ALTHOUGH not originally intended, it seems appropriate to add here a few words on the third fundamental conception of physics—*mass.*

The presupposition for measuring length and time is, as we have seen, the acceptance of an absolutely solid (immutable) body, and of an immutable process. Hence the possibility of formally ("kinematically") describing motion, whose characteristic quantity, *velocity,* for instance, is defined as a length divided by a time. Nevertheless, these two fundamental suppositions alone will not carry us very far.

The consideration of the larger or smaller effort necessary to change the position of a body, most likely developed naturally and

early. For instance, a primitive experience is that a stone will remain in the same place despite a gale which causes leaves on the ground to move great distances. The apparent cause of the fact that bodies persist in their state of rest or of (translatory) motion, is that which we call *inertia* or *inert mass.* Experience shows that every body endowed with inert mass—at least when it is sufficiently distant from other bodies—continues to move in a straight line, though perhaps with the velocity zero (i.e. it may remain stationary). As is well known, this observation of Galileo's constitutes Newton's first law of motion. It has been remarked that no reference is here made to the co-ordinate system to which the rectilinear path is to be referred, and that this implies a great difficulty of comprehension.[1] Yet, as a matter of fact, the rectilinear path may be referred to any

[1] *Cf.* C. CHWOLSON, *Lehrbuch der Physik I*, Braunschweig, 1902, p. 74.

inertial co-ordinate system that is convenient.

Such a system will be formed if at a given moment, and say as by an explosion, a number of particles are thrown off from a given point. They will then continue to move along straight lines, three of which will together form an *inertial* co-ordinate system. If a particle moves in a rectilinear way with reference to such a co-ordinate system, we know that it also moves rectilinearly with reference to every other inertial system. According to mechanics, a rectilinear motion of two bodies is *relative*—that is, independent of every translatory motion which they have in common (relativity of mechanics). If, for instance, a man fires with a rifle at a target, the result will not be altered whether the shooting takes place in a stationary building or on board a ship which moves with constant speed. The result will be the same no matter whether the aim is parallel with, or

at right angles to, the direction in which the ship is moving.

As experience has shown, this mechanical relativity is valid only in the case of rectilinear motion, and not, for example, for a rotatory motion. In this case it appears, as is well known, as if an "absolute space" existed, so that every rotation with reference to space might perhaps manifest itself. We may call to mind, for instance, the famous pendulum experiment of Foucault, by which the earth's rotation may be demonstrated at any time.

This conception of an "absolute space" and the enigmas which arise through it have caused a great deal of brain-cudgelling. Suppose that in our imagination we let all the masses extant in the universe unite so as to form one single gigantic sphere in space, outside which nothing is to be found. We have then a certain right to doubt whether the rotation of this sphere, relative to the surrounding empty space, would give

rise to any physical result, such as flattening on account of centrifugal force, turning of the plane of a Foucault pendulum, or the like. If no fixed point outside the sphere were given, rotation or rest with reference to the empty space would be indistinguishable. To many persons, therefore, it appears repulsive to ascribe an influence to "absolute space."

This difficulty, however, seems to me to be considerably exaggerated. In reality, we need only imagine a single external particle, whose mass may be infinitely small as compared with that of the sphere; therefore in itself unable appreciably to alter the physical conditions, and consequently in itself quite negligible. Let this *index particle* be ejected from the sphere, and, for the sake of simplicity, say in a radial direction. Now, if we still assume the validity of the laws of mechanics, its path will be a straight line, *constituting an inertial co-ordinate or reference axis, which in*

general will permit us to decide geometrically whether rotation is occurring or not. We might, for instance, fire a projectile exactly in the direction of the plumb line. As we assumed external disturbances to be excluded, this projectile would drop down again on its starting-point if the sphere had no motion of rotation with reference to the "absolute space." On the contrary, if the sphere is rotating, a deflection will take place, which is in proportion to the speed of revolution. The experiment would be still more precise if a high and light tower (of negligible mass) is assumed to be used. From its top (say inside a closed tube) a precision plumb could be suspended, and a ball dropped. The distance between the striking-point of the ball and the plumb-point would be proportional to the speed of rotation relative to the "absolute space." Or, of course, we might use the turning of the pendulum plane or the gyroscope.

Therefore, as a matter of fact, we are

forced to acknowledge that, even supposing *practically* all matter to be concentrated into one single sphere, things would behave as if the Newtonian " absolute space " did exist—so far as rotation is concerned.[1] In other words, Nature behaves as though an " absolute rotation " really would exist.

Yet it must be admitted that there is full justification for raising the objection against this Newtonian absolute space (almost materially conceived) that " every point in space is perfectly identical with every other point; no point possesses anything which might be able to make it distinctive in our mind."[2] How are we to escape this dilemma?

According to the present writer, the fact of the matter is simply this. Obviously

[1] The present writer thus cannot avoid coming to the conclusion that Mach, and after him Einstein, have made a mistake in asserting that under the supposed circumstances rotation would have no physical effect.

[2] E. Wiechert, *Die Kultur der Gegenwart, Physik*, p. 40, Leipzig und Berlin, 1915.

space in itself cannot be said to possess an "absolute" character. The character of the conception of space is to be something geometrically undetermined or merely an expression of the fact that co-ordinate systems may be applied anywhere and in any directions. The possibility of "absoluteness" enters with the conception of an *inertial body* (mass, mass-particle); or, more correctly, with the experience previously referred to, that a mass-particle, when not affected by other masses, does continue to move along a straight line. For the mass-particle considered, and unaffected by the outer world, the direction of its straight path must be said to have a decidedly *absolute character.* The mass-particle progresses in the otherwise undefined, empty, characterless space, *as if an absolute,* uniquely possible, sharply defined direction existed and was marked out for it. We may say that the moving mass-particle, on account of its inert mass, is

ruling its own way along a straight line. In other words, the very *antecedents* of the mass-particle itself—say two points which it has passed successively—give a certain absolute character to its direction.

To me, therefore, it seems nugatory to worry about the question why *space*, in spite of its obvious indefiniteness, appears to have such an absolute character. There is more reason to meditate on the question why inert mass, with its property of creating directions in space, is a characteristic of matter.

It is exclusively this property of inertia in matter which causes space to behave as an "absolute space"; should this term be found not quite satisfactory, it at least expresses the real facts.

This exceedingly important question might, of course, call for a more exhaustive analysis than given in this short exposition. We shall only summarize what has been said by stating the following results.

If it is objectionable for us to state that a body is rotating with reference to the surrounding empty space, to which we thereby assign an "absolute" character, the reason for this adverse impression is simply that the fact has been overlooked that by expelling a mass-particle, be it ever so small, from the body, one is, so to speak, able to rule a straight line in space. This line may be used geometrically as an index to the state of rotation of the body, and thus physical effects of the rotation will be easily apprehended.

We have to add that concerning the *inert mass of bodies*, this kind of mass is known to be quantitatively defined as the proportion between a *force* acting upon the body and the *acceleration* thereby caused—that is, the increase or decrease in velocity per unit of time.

Regarding the conception *force* it is difficult to say what definition is to be preferred. It seems most rational, perhaps, to refer

to the primary experience embodied in the existence of *elastic bodies*, and to state something like this: if, under otherwise fixed conditions, an elastic body is subject to elongation, experience shows that something inside the body is striving to restore the original length, and to this tendency, proportional to a small elongation, we give the name of *force*.

Further, experience shows us that every body exercises a force, or a tendency to reduce the distance, upon every other body. The earth, for instance, attracts a stone with a definite force. The greater this force, the greater is said to be the *gravitational mass* of the body. Experience shows that in a vacuum all bodies fall with the same velocity. From this we conclude that for all bodies inertial mass is proportional to gravitational mass; we are even able to regard *inertial mass* and *gravitational mass* as identical in value.

This equality enables us to establish a

standard unit for mass in the same way as for length and time.

The identity between inertial and gravitational mass is an exceedingly striking fact. It cannot so far be said to have been satisfactorily explained by science. It seems to me that a deduction given by the theory of relativity of this identity would constitute one of its most interesting achievements.

Since mass is defined by a mass-standard assumed to be immutable it is a natural consequence that physicists have assumed the mass of a body to be a quantity strictly constant under all circumstances. It may be asked, however, how far this constancy of mass is a proposition necessary to classical mechanics. The answer would be along the following lines. If we assume, as has frequently been done, that the attraction of masses is propagated with infinite velocity, it follows that the mass of a body may appear as a strictly constant quantity.

On the other hand, if we assume that the attraction is propagated with a *definite* velocity, say, for instance, that of light—and this assumption does not, at least in itself, clash with classical mechanics—then the following will be true. In order that the velocity of a moving body may increase, as in the case of a falling stone, whose velocity increases on account of the earth's attraction, it is absolutely necessary that the attraction should progress with a velocity greater than that of the body itself. Now if the velocity of the body approaches the velocity with which the attraction is propagated, the acceleration—or velocity increase—of the body must continually diminish. Thus, according to the definition, the inert mass of the body will continually increase. If finally the body moves with the same velocity as that of the attracting force, no further increase in velocity can take place. This implies that the *inert mass* as defined will be *infinitely great.*

It is, therefore, in full conformity with classical mechanics, provided that a finite velocity is assumed for the propagation of attraction, to say that the mass will be found to increase with increasing velocity, and will at last become infinite. Consequently it is in no way, as has been generally assumed, a special prerogative of the theory of relativity to infer that variations of mass become perceptible when velocity approaches to that of light.

AMALGAMATION OF THE FUNDAMENTAL UNITS

WE have seen above how simple and clear the classical conceptions of length and time are in themselves—at least they do not hold us in bonds of ignorance—and how at the same time they are based upon the conventional assumptions of *immutable body* and *immutable process*. The truth of these assumptions is beyond the possibilities of *proof*, though they tally closely with experience.

The question now arises, is it beyond the bounds of possibility that these definitions of length and time may in spite of all be incorrect, so that they need to be revised on the ground of keener observations? No; a definition can only be said to be

incorrect when it is in opposition to previously accepted fundamental conceptions. In fact, a logically constructed definition of a fundamental conception cannot be said to be incorrect; it can only be said that perhaps it is not very practical, not very fruitful. But in such a case, before a change is accepted, the new definitions must be shown to be more practical, more convenient for use.

If, for instance (as referred to later on), we were to reject, as Lorentz has suggested, the old definition of length and regard length as a function of motion—thus depriving geometry of its Palladium of exactitude—then we have the right to demand that extraordinary advantages should be shown (far greater than that of saving certain hypotheses concerning the nature of light, which we shall mention later on), or else that there is absolutely no other escape available.

If, for example, Einstein rejects the con-

ception of simultaneity in itself, and instead tries to make simultaneity depend upon certain conditions of motion, then it is obvious that exceedingly weighty advantages must be adduced before such a complication of conceptions is to be accepted by a critical natural philosopher.

But, as one might ask, does the linking up of length and time [1] mean a step forward perhaps as great as the linking up of electricity and magnetism ?

We may answer that the choice of units, fully independent of each other, for length and time, is induced by purely practical measuring purposes. It would not be at all difficult to refer the unit of time to the unit of length without considering any new phenomenon. Having defined the meter as length-unit, so as to make it one ten-millionth part of the earth's quadrant, we

[1] Compare Minkowski's well-known and presumptuous words that from now on space and time [as separate entities] have fallen to the level of shadows only, etc.

could possibly define the time-unit as the time required for a point on the equator to proceed one meter with reference to a given straight line drawn from the earth's centre to a fixed star. But this would be advantageous only if some new and as yet unknown natural law had been discovered combining the quantities in question.

The present writer may be permitted to state that of the knowledge gained during his student life, nothing has ever given him the impression of human *wisdom* so much as the selection of three independent, fundamental units, directly available for measuring length, time, mass.

We must now enter into a brief explanation of the motives which have led men to introduce such a remarkable alteration or fusion of these conceptions. For this purpose we must first glance backward at the historical development of our knowledge of the nature of light; a knowledge, as may be said at once, that is defective.

OPINIONS ON THE NATURE OF LIGHT

DEMOCRITUS, the brilliant creator of the atomic theory, considered that the sensation of light is caused by minute particles being thrown off from an object and reaching the eye of the observer.[1]

Towards the end of the seventeenth century Huygens brought forward his theory that light is caused by a wave-motion (which, by the way, he supposed to be longitudinal) in an ether that permeated everything.

Newton opposed this *undulatory theory*—chiefly it seems on account of the difficulty of explaining propagation in a straight direction—and expounded his *emission*

[1] *Cf.* F. CAJORI, *A History of Physics*, New York, 1917.

theory (or corpuscular theory). This prevailed until about 1825, when the splendid work of Young, Fresnel, Malus, Brewster in the realm of higher optics caused it to be generally abandoned. A decision was reached in May, 1850, when Foucault proved that the velocity of light is not as great in water as in air. This was in conformity with the undulatory theory, while Newton had assumed that the velocity must be greater in a more dense medium. This "*experimentum crucis*" was fatal to Newton's emission theory; and it is generally regarded as illustrating one of the mistakes to which even the very greatest authorities are subject at times.

The present writer, when newly appointed professor of physics, asked a student who was being examined why the emission theory is not satisfactory. The student gave as a reason that the emission theory demands that the velocity of light should be greater in a denser medium, which is not

the case. The answer was, of course, entirely correct, but it struck me that it is not possible that the velocity of light, according to the emission theory, *must* be greater in the denser medium. The position really is as follows. By means of Huygens' principle, which signifies that every illuminated point may be regarded as a new starting-point for radiation, the undulatory theory gives us an elegant construction for the refraction of light towards the normal in the denser medium. This principle being alien to Newton, he assumed instead a kind of attraction between the " matter of light " and the real matter—which would cause an increase in the velocity of light in the latter. But it is easy to find that *if* we assume the Huygens principle to hold good also in the case of the emission theory, *this will give refraction in exactly the same manner as the undulatory theory, with lower velocity in the denser medium.*

Thus it must be admitted that the emis-

sion theory, in the form that Newton gave it, must have been rejected; *Foucault's experiment, however, does not exclude an emission theory for which Huygens' principle holds good.*

There may be an objection that it is very difficult to imagine such an emission, and this objection is not unfounded—though, indeed, nowadays little stress is laid upon the lucidity of assumptions made. However, the applicability of Huygens' principle to the emission theory ought not to involve any insuperable obstacle. We shall return to this on page 81.

The victory of the undulatory theory over the Newtonian theory turned later into a veritable triumph, thanks to perhaps the most splendid achievement ever attained in mathematical physics—Maxwell's electromagnetic theory of light, founded on Faraday's views. Like the undulatory theory, this was based on the assumption of a "quasi-solid, universal ether."

Marked difficulties arose, however, also in this theory. Apart from the fact that it had to be thoroughly reformed by H. A. Lorentz in order to comply with the electron-conception of the present day—which was altogether unknown to Maxwell—a first difficulty arose in connection with the explanation of the aberration, or deviation between the direction of a telescope and the straight line from an observed star. This, as was shown by Bradley, is caused by the fact that the velocity of light is not infinitely greater than the velocity of a point on the surface of the earth. The emission theory gave a very plain, exact account of the aberration, while a full explanation of this phenomenon from the standpoint of the undulation hypothesis defied the penetrating investigations of Fresnel, Stokes, and others, and even those of Lorentz. These difficulties are associated with the question to what extent the supposed ether—in opposition to what

was originally assumed—must be regarded as carried along with or convected by the earth in its motion.

In order to settle this question, Michelson carried out his famous experiment (later repeated, with Morley, in 1887). Its negative result has probably been spoken of more than any other positive one. The supposed *ether-wind* caused by the earth's rotation proved to have no influence on the rays of light.

Lorentz bestowed great labour on the thorough discussion of this experiment (1892), and declared that he had "at last arrived at only *one* conclusion"[1]—namely, that the *length* of *a solid body*, the structure of the apparatus, was altered as a result of the rotation. Apparently he is thereby rejecting the very fundamental conception of geometry. Strangely enough, at the

[1] H. A. LORENTZ, *Abhandl. uber theoretische Physik*, I, 2, p. 443, Leipzig, 1907. ". . . schliesslich nur *einen* Ausweg gefunden."

time this proposition was put forward (which from a scientific point of view, in spite of its modest formulation, is somewhat anarchistic), Lorentz never mentioned the fact that *the emission theory fully explains the experimental results* in question. Five years earlier, as we have seen, he had pointed out that this gave the solution in the case of aberration.

It is easy to see why. On the emission-hypothesis the question is much the same as that of firing at a target on a ship, mentioned on page 33. Whether the aim coincides with that of the ship's rectilinear motion, or is at right angles to it, makes no difference to the result—provided there is no effect of wind. This is on account of the mechanical relativity.

But, on the other hand, it is easy to understand why the important contribution Lorentz has made towards perfecting the Maxwell theory, should restrain him from falling back upon an emission theory long

abandoned. The same expedient, the contraction hypothesis, was, by the way, also suggested by FitzGerald (1893).

Michelson's experiment can be regarded with the same right as Foucault's experiment, as an *experimentum crucis* between the ether-undulatory theory and an emission theory. On the undulatory theory an effect should be found; on the emission theory no effect should occur. Surprisingly enough, the experiment must be said to have decided in favour of the latter. The only natural consequence—at least for an impartial experimental physicist—would have been to investigate how the existing theories of light must be modified; the emission theory so that periodicity may be introduced, and Huygens' principle made applicable to it; or the electromagnetic theory so as to be equivalent to an emission in space.

The proposal of Lorentz and Fitzgerald, involving an obvious alteration of the con-

ception of length, which we have seen to be clear and distinct, must have appeared to many scientists as altogether too daring, and, at the same time, too unfruitful.

It was A. Einstein [1] who renewed the question in his famous work *Zur Elektrodynamik bewegter Körper.* He pointed out at the beginning that the mutual action between a magnet and a conductor, according to Maxwell's electromagnetic theory with its assumption of a solid "ether," must differ according to which of the two is regarded as a moving body, while the physical action—the production of an induced current—is exactly the same in both cases. The unsuccessful experiments, especially Michelson and Morley's, to prove a motion of the earth with respect to the "ether," led to the surmise that as in mechanics all rectilinear motion is relative—independent of any "absolute co-ordinate system," or "ether"—so also in optics

[1] A. Einstein, *Ann. d. Phys.*, **17**, p. 891, 1905.

only relative motions are of consequence, and not motion with respect to the "ether," as assumed by Maxwell's theory. It is this fundamental idea which Poincaré [1] characterized as the *relativity-postulate*, and there is no doubt that the establishment of this conception is an entirely legitimate aim.

Let us repeat that this theory of relativity makes its appearance at once provided one accepts the assumption of emission. Strangely enough Einstein, like Lorentz, omitted any mention of this. The reason was very likely the same. To everyone who has specialized on Maxwell's theory, it must seem atavistic to return to the Newtonian theory of emission; and no other emission theory was at hand.

The second assumption made by Einstein as a basis for his special *relativity theory* is that the velocity of light in space must under all circumstances appear as a con-

[1] H. Poincaré, *Rendic. del Circolo Matem. di Palermo*, 1906, **21**, p. 129.

stant velocity *c*, independent of the motion of the source of light itself. This must be regarded as much less legitimate. He says: "We conclude that in accordance with experience the quantity *c* is a universal constant."

To this it may be objected that the results of experiment which we have so far gathered in this realm are so few that the least we could have desired was to have the main facts clearly stated.

That the velocity of light, that is, the velocity measured with reference to the source of light, is in itself a natural constant of the highest importance will be denied by none; but it is far beyond experience to assert that no higher velocity could exist in Nature.

At any rate, this velocity of light, assumed to be invariable under all circumstances, is used by Einstein as a basis for his definition of simultaneity. We may say that he substitutes a ray of light with a finite velocity

of propagation for the absolutely rigid connection (see p. 26) in the classical conception of time, which implies an infinite velocity of propagation. We need not here enter into the question how far the resulting complication of time is consistently or, as it has been asserted, not consistently developed in the consequent doctrine. The radical alteration of our fundamental conceptions is at any rate a fact.

We have seen, however, that the scientists mentioned have in their fundamental papers left out of consideration the possibility that exists of returning to the emission theory—though not, of course, in its Newtonian form.

RITZ'S OPINION

THE merit of being the pioneer in pointing out clearly this possibility of preventing the destruction of the fundamental conceptions of physics is due to the brilliant Swiss physicist, Walter Ritz (born 1878, died 1909). During his short span of life he found time to open up new and important avenues in spectroscopy, and, among other things, to enrich mathematical physics with a new and exceedingly fruitful mathematical method.[1]

[1] It is interesting to read Poincaré's opinion on the mathematical importance of Ritz's methods. For the solution of somewhat similar problems of mathematical physics, FREDHOLM (Stockholm) has given a general and exact method; but the difficulties of applying this to

Ritz proved in a splendid way the usefulness of his method by giving a full mathematical solution of the nodal-figure problem of Chladni, which had defied the efforts of all previous scientists.

During his few years of activity, the singularly gifted young man had the opportunity of being in intimate contact with a number of the most prominent scientists of the time—Hilbert, Voigt, and Minkowski of Göttingen, H. A. Lorentz of Leyden, Cotton and others in Paris. One of his papers was published in conjunction with Einstein. Exceptionally free from dogmatic prejudices, gifted with a trustworthy power of discernment, with skill in experiment, but

numerical calculations are almost insurmountable. Ritz's method is certainly not so profound, but is well adapted for calculations which may be carried out to any desired exactitude. Even in a purely analytical respect Ritz's method yielded results which fully correspond with Fredholm's, and which appeared to Poincaré to admit of generalization. (W. RITZ, *Gesammelte Werke. . . .*)

having above all a thoroughly trained and expert knowledge of the electromagnetic theory of light and of other mathematico-physical problems, Ritz must be admitted to have been exceptionally fitted to judge the value of the relativity theory launched by Lorentz, Einstein, and Minkowski. His opinion is well worthy of widespread attention, which was rendered difficult by his untimely death. A brief summary of his ideas will therefore be given here.

In 1908 Ritz published an extensive memoir (occupying 130 pages in his collected works),[1] which contained a critical scrutiny of electrodynamics in general. In this memoir stress is laid on the fact that H. A. Lorentz's modification of Maxwell's electrodynamical theory had attained an extraordinary importance for physics—perhaps also as a foundation for mechanics.

[1] W. Ritz, *Gesammelte Werke, Oeuvres publiès par la Société Suisse de physique.* Paris, 1911.

Consequently, a vigorous criticism of its fundamental principles is very desirable; a criticism that has been silenced on account of all that the theory has rendered to physics. Maxwell's equations had attained "to such a degree the character of axioms, that one hardly hesitates to sacrifice almost all other physical axioms for their sake. A *strange fate for a theory* which was almost unrecognized during the lifetime of its creator," said Ritz rightly, in his inaugural lecture [1] a short time before his death.

As experience has shown that optical phenomena likewise depend only on relative motion, and as the Maxwell-Lorentz theory, based upon the conception of the ether, does not give this relativity, some change must be made.

To suppress the conceptions of a solid body and an invariable mass; to modify

[1] *L.c.*, p. 513.

the principles of dynamics, admitting the theorem of the parallelogram of forces as a first approximation, valid at low velocities only ; to alter the conceptions of time and simultaneity—" to accept such complications would be deplorable for our economy of thought. Instead of altering dynamics, I consider that it is the ether-hypothesis which ought to be abandoned. This conclusion by no means involves a return to action at a distance."

What one has to return to is the conception of light as an emission : " fictitious particles are constantly being expelled in all directions from an electric charge." Such a supposition satisfies the claims of relativity.

Ritz recurs to these ideas in two later papers, in *Scientia*. A well-known paradox in Einstein's theory is expressed in the following lucid way : " If two electrons are ejected from a particle of radium in opposite directions with a velocity of 250,000 kilo-

metres per second, they attain a relative velocity not amounting to 500,000 but to about 296,000 kilometres per second. That is to say the meaning of these expressions is totally altered. In order to save the ether, and Maxwell's equations—or at least the remaining fragments of them—will the physicist readily consent to accept such complications ?"

It is deplorable in the extreme that this ingenious investigator did not have time to work out the conception of emission in electrodynamics, as was his intention, in detail and in a manner satisfactory to himself. He was fully aware, though, that however comprehensive a form might be given to his ideas, they would be accepted only with great reluctance by his contemporaries. How long the negative position of the "contemporaries" will be maintained is another question.

It should be added that, independently of Ritz, the three American scientists

Tolman,[1] Kunz,[2] and Comstock[3] have asserted (in 1910) their preference for the conception of emission.

[1] C. TOLMAN, *Phys. Rev.*, **30**, 1910, p. 291; **31**, 1910, p. 26.

[2] J. KUNZ, *Am. J. of Science*, **30**, 1910, p. 1313.

[3] D. F. COMSTOCK, *Phys. Rev.*, **30**, 1910, p. 267.

RETROSPECT

AT the beginning, in order to penetrate to the foundations of the matter, we examined the classical conceptions of length, time, and mass. We learned, if indeed we were previously ignorant, that they form clear, well-defined conceptions which by universal agreement have been chosen as a fundamental basis for all exact investigations of Nature. These conceptions must not be altered recklessly. A few decades ago no one would have thought of modifying such fundamental ideas. For the economy of scientific thought it is desirable that alterations, so far as they are necessary, should affect

conceptions which are not of a primary order.

As a matter of fact, the prevailing theory of light, which assumes a more or less stationary ether, demands that the motion of the source of light and of the receiver, with reference to the supposed ether, should influence optical phenomena. But experimental evidence has proved that no such influence exists. This means that in optics, just as in mechanics, all translational motion in space is *relative.* In consequence, *some alteration* must *indisputably* take place if a theory of relativity is to result—that is, if the "relativity postulate" is to be valid also in the domain of optics.

Now an alteration may take place according to either of the two following alternatives:—

Alternative I.—Abandonment of the Maxwell-Lorentz theory of light as founded upon the ether; that is, abandonment of the conception of a stationary medium,

whose changes or "elastic deformations," though actually incomprehensible to thought, are supposed to cause the propagation of light. Instead, the assumption that *light is an emission into empty space.*

It will, of course, take a considerable amount of work once again to reform the electromagnetic theory so as to bring it into agreement with the new idea of the mode of propagation of light. Unfortunately Ritz died before he had time to carry out such a revision. But it is scarcely to be expected that the readjustment will complicate the theory. We may rather expect a simplification, in that we have to deal with (so to speak) one variable quantity less—the motion with respect to the ether disappearing.

Alternative II.—Retention of the Maxwell-Lorentz theory of light and its ether hypothesis without change. Instead, an alteration must be made in our funda-

mental conceptions of length, time, mass, and so forth; so that the mathematical operation which Poincaré has called the Lorentz-transformation, may be performed and the claim of relativity satisfied.

The second alternative has been chosen by the author of the theory of relativity. From the standpoint of a specialist in optics or electromagnetism, this is decidedly the more sympathetic choice. Alteration of the fundamental definitions of length, time, mass, and so forth, is made with a few strokes of the pen. Their essentially simple content makes them easier to alter than the organically constructed theory of light. The mathematical transformations are also easily carried out; and then the beautiful Maxwell-Lorentz theory still holds good.

But it was the first alternative to which Ritz decided that we should return, in spite of the radical readjustment of the theory of light thus rendered necessary.

Considering the matter not only from an optical but also from a standpoint of general physics, the labour involved as a consequence of the alterations of the fundamental conceptions is far greater and much more extensive; and from a philosophical point of view, this is the case to a still higher degree.

Let the following analogy serve to make the matter quite clear. The wheel-gauge of a new, ingeniously designed locomotive is found to be too wide for use on the state railway track. Two alternatives are at hand for making it serviceable:—

Alternative I.—The wheel-gauge of the locomotive is to be altered so as to suit the existing railways.

Alternative II.—The railway lines are to be re-laid so as to fit the larger wheel-gauge of the locomotive.

The inventor and his friends, well aware of the design, admirable in all details, and perhaps considering also the advantage to

be gained by widening the track, will of course prefer the second alternative. The engine must not be altered; how much simpler it would be to move a few rails! Of course, the designers may be right, if the question is only that of a first trial run.

Yet it is quite certain that those who bear the responsibility of a steady development of the railways will choose the first alternative, and this for *economic reasons.* The cost of relaying the whole line, with the additional alteration of all existing rolling-stock, including all older locomotives—would certainly amount to sums of quite different magnitude from those necessary for the alteration of the new engine. The inventor would probably receive the answer from the railway board that for economical reasons they could have nothing to do with the new locomotive, unless it could be adjusted to the existing conditions of the line. This, perhaps, even while it is

acknowledged that the new rail-gauge might in itself present definite advantages.[1]

The brilliant designers in our case (Maxwell and Lorentz) will also—so it seems to me—have to put up with their creation being readjusted so as to conform to the existing lines of thought, the established conceptions of length and time. The bold attempt to create a system of thought with a broader gauge (Lorentz and Einstein) may in itself be ever so worthy of recognition, yet human thought will certainly find it *extravagant* on the whole. We shall,

[1] This is a by no means loosely constructed example. When Stephenson first constructed his new locomotive, he was forced to fix the wheel-gauge in accordance with the existing tracks of the coal mines (4 feet 8½ inches, or 1435 mm.), though a wider gauge would have been desirable. In spite of a controversy lasting many years (known in England as "the Battle of the Gauges") between this and a wide gauge, the former was adopted, on account of its priority. It is now said to be in use in three-quarters of all the railways of the world. (From *Uppfinningarnas Bok*, ix, p. 108, Stockholm, 1909.)

however, certainly be able to profit by certain constructive details.

I think the core of the matter has now been opened sufficiently to allow everyone to see the question clearly.

Two further points may yet be considered in the sequel.

POSSIBLE CONQUESTS OF THE THEORY OF RELATIVITY

WE are far from denying a heuristic value to the great design of Einstein. I cannot agree with the view, recently put forward by a prominent theorist, that the effects predicted by the theory of relativity are too slight to call for general interest. Independent of whether the difficulties of measurement entailed are great or small, every mastery of a new natural phenomenon must be regarded as an exceedingly important matter.

Accordingly, great credit is due to Einstein for having drawn the attention of astronomers to a problem of such profound importance as the deflection of a ray of

light in passing near the sun ("the Soldner effect").

Let us suppose that this prediction of Einstein's should prove correct,[1] in spite of the numerous sources of error; and that the same should be true with regard to the famous motion of the perihelion of Mercury's orbit. This would naturally constitute a great triumph for the penetrating mind of the discoverer; but it would by no means stand as evidence that his theory is the only right one, or the best one. We might draw attention here to the remarks made by Poincaré in *Science et Hypothese,* where he states, following Maxwell, that if one explanation of a phenomenon is found, there are also an infinite number of other ones to be found!

Such effects as the ones mentioned will probably be deduced in a simpler way in

[1] While this translation is in progress, reports from the Lick Observatory give us to understand that this seems to be the case.

the future. As a matter of fact, according to the Newtonian emission theory of light, a deflection at least half as large is to be expected for a ray of light passing in the sun's vicinity. Such a deflection had actually been calculated by J. G. von Soldner [1] 110 years before Einstein. As for the perihelion motion in question, almost exactly the same value is deduced on a theory derived from Riemann.[2]

The theses contained in Einstein's later "general theory of relativity" are undeniably, in themselves, very skilful and imaginative. But here likewise we find a contempt, which is carried extremely far, for our hitherto accepted modes of reasoning. Up to the present, for example, it has been regarded as an important matter that we might consider say two hydrogen atoms by themselves, at so great a distance from other atoms that the influence of these

[1] *Ann. d. Physik*, **65**, 1921, p. 593.

[2] G. Bertrand, *Comptes Rendus*, **174**, 1922, p. 1687.

may be disregarded. According to Einstein, this would no longer be justified, in so far as the attraction which exists between two atoms is assumed by him as dependent on *all other* atoms—even though infinitely distant. If the outside atoms were to be eliminated, the attraction between the two atoms would no longer exist.

Such a view cannot be regarded but as a challenge against the modes of thought applied in all previous physical reasoning. Action at a distance was long ago rejected by physics; this is action at a *super*-distance.

THE POSSIBILITY OF AN EMISSION THEORY

FINALLY, we ask whether the conception of *emission* may in itself cause any great difficulties to our mind. History, as we have seen, points rather to the contrary. Democritus and Newton, both masters of clear, tangible thinking, assumed the idea of emission as the most natural conception.

We must, however, examine more closely the objections raised against the emission hypothesis. We cite these from an authoritative, scientific exposition of the theory of relativity.[1]

[1] W. PAULI, Jr., *Enzyklopädie d. mathem. Wissenschaften*, p. 549, Leipzig-Berlin, 1921.

First, we may state that when talking of an emission hypothesis, we mean every hypothesis according to which the velocity of light is influenced by the motion of the source of light.

With regard to the *reflection* of light, it can hardly be asserted that there is any marked difficulty from the standpoint of the emission theory.

The *refraction* of light might be somewhat more difficult since its deduction certainly demands (as has been pointed out above) that Huygens' principle be accepted as valid. That is to say, every illuminated point may act as a source of light. The validity of this may, at first sight, appear less obvious on the emission hypothesis. But it is easy to realize that an analogy to Huygens' principle is valid even in the case of flowing matter. As in the example of a water-chute, suppose water to flow down in a thin sheet over an inclined plane. The propagation will then take place along

parallel straight lines. Now consider that a partition, provided with a vertical slit, is placed vertically across a horizontal line of the inclined plane; in consequence of this *barrier* the slit will act as a *source* for the lower part of the plane, and divergent lines of current will issue from it—in analogy with Huygens' principle. As a matter of fact, we may say that even in optics Huygens' principle has validity only when barriers are present. We may obtain a notion of its validity in the case of emitted light in different ways. A simple assumption would be that at the barrier—which may, for example, be a grating, or screen with regularly spaced parallel slits—the incoming parallel rays of light are reflected also by the edges of the slits, so that diverging beams of light (of lessened intensity) would issue on the opposite side.

Without entering further upon such somewhat specific suppositions, we may admit that the adoption of Huygens' principle

need by no means be excluded by the emission hypothesis.

We shall not discuss here the question of how the emission theory, without any new or special assumption, is able to deduce the convection coefficient in Fizeau's experiment with a ray of light passing through a flowing liquid. Menges[1] has recently shown that the coefficient in question (originally calculated by Fresnel), with a correction introduced by Lorentz, may be deduced in a way wholly different from that of Lorentz, and one which seems to be quite in agreement with the emission theory.

Regarding the *Doppler-effect*, or the variation in frequency which is caused by displacement in the direction of the light, it may only be stated that this is derived in an extremely simple manner from Ritz's standpoint.[2]

[1] C. MENGES, *Comptes Rendus*, **175**, p. 574, 1922.

[2] This is adequately acknowledged; as by W. Pauli,

Finally, we come to the main objection against the emission theory; namely, the conclusion reached by the Dutch astronomer de Sitter [2] implying that the orbits of certain spectroscopic double-stars are in conflict with the assumption that the velocity of a moving source of light should be added to the velocity of the light itself, as the emission theory demands. For double-stars, possessing "in reality circular orbits," a certain eccentricity of orbit should be recorded if the velocity of light could change in magnitude owing to the motion of its source.[2]

Against this, however, the German astronomer Freundlich [3] has raised the point that the conclusion is entirely dependent on the assumptions made for the calculation of the "real" orbits. The same investigator also lays stress upon a fact discovered

[1] W. de Sitter, *Phys. Zeitschrift*, **17**, p. 429, 1913.
[2] *Cf.* W. Pauli, Jr., *l.c.*
[3] E. Freundlich, *Phys. Zeitschrift*, **14**, p. 385, 1913.

by *Miller Bar* (1908), that in the majority of double-stars then observed (23 out of 26) an eccentricity, oriented towards the solar system, has been observed. This fact was discovered independently of any view concerning the nature of light. Such an orientation, which of itself must be regarded as very improbable, is demanded by the fundamental assumption of the emission theory that the motion of the source of light does affect the velocity of light. In opposition to this result de Sitter[1] has adduced another statistical observation, namely, that the most distant among the observed spectroscopic double-stars exhibit only a slight eccentricity, while according to the emission theory a marked and observable eccentricity should occur. Here, however, one who is not an astronomer misses an examination of systematic sources of error which may be involved in the perhaps simpler observa-

[1] W. de Sitter, *Phys. Zeitschrift*, **14**, p. 1267, 1913.

tions of distant double-stars possessing only slight eccentricity.

Further, we must not leave out of consideration a fact which lies within the bounds of possibility, that a ray of light passing through space outside the earth, with a velocity (in relation to the earth) which is greater than the normal one, might be considerably retarded in its unavoidable passage through the earth's atmosphere.

Without taking any definite standpoint with regard to these interesting special problems of astronomy, it is impossible to dismiss the conclusion that the main objection to the emission hypothesis is founded on facts which are too uncertain to justify its claim as a really serious obstruction. From the preceding we may also see that the other objections to the possibility of the emission hypothesis are hardly decisive.

The final summary will thus be that the basis for dismissing Ritz's conceptions has

been a matter of taste rather than a matter of fact; and regarded as an excuse for a wholly new and revolutionary theory of physical reality, the reasons against the emission theory cut a very poor figure.

FURTHER REMARKS ON THE EMISSION THEORY

ACCORDING to what has been said, it is of the utmost importance for physics to develop further the emission theory. Without any great pretence, the following reflections are meant to contribute in some way to the discussion of how a model of light radiation may be conceived on the basis of emission.

At the start we take Ritz's [1] fundamental assumption, as previously mentioned: "fictitious particles are constantly ejected in all directions from an electric charge." They are assumed to progress rectilinearly (with respect to the charge) with the constant velocity of light. According to Ritz,

[1] W. Ritz, *Gesammelte Werke*, p. 321.

the fictitious particles must be regarded merely as a " concrète presentation of kinematical and geometrical data " (" représentation concrete des données cinématiques et géométriques "). According to the fundamental conception, the emission thus issuing from every electron may be regarded as a continual succession of *explosions taking place at short intervals.* Consequently, according to Ritz, an electron may be viewed as the *centre of an iterated explosion.*

This may appear paradoxical ; as, however, at the present day very little is known about the constitution of the electron, it may be that the assumption in itself contains no absurdity.

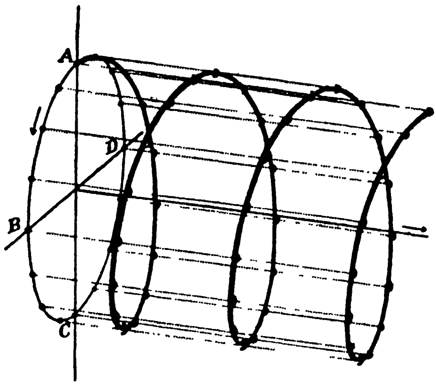
It follows from this assumption that an electron in a state of rest cannot produce anything equivalent to an emission of light ; what can appear is a *field of force.* Here we may conceive the situation to be roughly in accordance with the well-known theory of C. A. and V. Bjerknes, which gives

evidence that a field of force issues from pulsating spheres.

If, further, the electron is revolving in a closed, say circular, orbit, the following would take place. At a given instant a spherical wave of explosion will be radially propelled from a centre formed by the position of the electron at that instant. At the next moment of explosion, a new and similar wave is emitted, with the new position of the electron as centre; and so forth. Let us now, for the sake of lucidity, consider a distant point on the axis perpendicular to the orbit of the revolving electron. Call this first electron the *oscillator*. We may then in imagination cut a narrow parallel beam of radiation from the emitted waves; this beam is supposed to be limited by the orbit of the oscillator; and at the point considered on the perpendicular axis we may suppose a second electron, a resonator, to exist.

The constitution of the elementary beam

of light so defined would be schematically represented by such a figure as that given above. The orbit of the rotating electron is supposed to correspond to the path ABCD on the left; the peaks of the emitted waves will lie along a cylindrical spiral,



determined by the orbit. This constitutes a *discontinuous spiral in space, or helix.*

The present writer has previously had the opportunity of pointing out that even for the present theory of light, which assumes a continuous deformation of the ether, a cylindrical spiral constitutes the

most natural geometrical representation of a light vector.[1] It corresponds to what we call a *circularly polarized* ray. The pitch of the spiral corresponds to the wave-length of the light.

Such a helix may, without elaboration, be supposed to cause the other free, moveable electron—the *resonator*—to vibrate. The resonance vibrations might be regarded simply as a consequence of the principle of *action* and *reaction*.

Naturally, a certain phase-difference, or retardation, will take place in the movement of the resonator. Hence we shall have a lower velocity of light in a medium which is optically denser—that is, a medium which contains a great number of resonators.

[1] C. Benedicks, *Om geometriskt, spec. rymdgeometriskt åskådliggörande af komplexa storheter och deras användande inom fysiken.* (The representation of complex quantities in plane and solid geometry, and its use in physics.) *Tekn. Tidskr. Veckouppl.*, 1916, o. 313. The cylindrical spiral is the geometrical representation of the complex exponential function Ae^{+kxi}.

If the resonator-electron is acted upon ta the same time by another ray of the same intensity—similar, but twisted in an opposite sense,[1] it must then be influenced as if it had been struck by a ray *linearly polarized.* That is, it must execute linear vibrations. Such linearly polarized rays are, moreover, obtained directly for points situated in the plane of the oscillator.

Two such polarized rays of equal intensity, possessing a phase difference of one-half period, must extinguish one another. That is to say, they must produce the phenomenon of *interference.*

On the whole, the difference in action between the theory of such *emitted* rays and the present theory of light by *ether vibrations* will probably be very slight. In one special case, however, the difference appears to be very important. From the standpoint ot the ether undulatory theory (otherwise the Maxwell theory) there is no reason

[1] The mathematical expression for which is Ae^{-kxi}.

for assuming a discontinuity in the emission of light. From the standpoint of the emission theory, however, a *discontinuous emission,* in conformity with what has been stated above, *is to be regarded as a necessity.*[1]

Now as a result of Planck's quantum theory modern physics has been compelled to assume some discontinuity in the emission of energy, or of light; though it has not been possible to make this harmonize with Maxwell's theory. Here also Einstein's ideas on light-quanta deserve attention. Conceptions of this kind, though so far insufficiently elucidated, harmonize far better with an emission than with an ether theory.

Finally, as Ritz pointed out, the assumption of emission gives us a much more natural explanation of the occurrence of light-pressure than the assumption of transverse vibrations of the ether.

[1] At least in a radial direction.

SUMMARY AND CLOSING WORDS

AT the beginning we convinced ourselves that despite all assertions to the contrary the traditional fundamental conceptions of space and time are quite clear and vigorous. Since they enter into almost all human thought, it would be impractical to alter them, except perhaps in case of absolute necessity.

No such necessity can be said to have arisen so far.

The reason why Lorentz, Einstein, and others have wished to alter the fundamental conceptions of physics, can be said to reside in an exaggerated respect for the Maxwell-Lorentz theory of light in its present form, combined with a somewhat scant respect for the fundamental conceptions in themselves.

All the optical phenomena concerned receive a full, qualitative explanation through a possibility first pointed out by Ritz; that is, the abandonment of the obscure conception of the ether, which gives rise to all the difficulties, and the substitution of light regarded as an emission in space. This will necessarily involve a readjustment of the Maxwell-Lorentz theory.

The following argument would seem to show that such a readjustment is possible, and constitutes a link in the natural chain of development.

(1) The electromagnetic theory, as presented by Maxwell, was known to assume on the one side continuous ether-vibrations, on the other an electricity-continuum. In spite of its elegance, the theory gave no quantitative agreement with reality.

(2) The readjustment undertaken by Lorentz implies that he substituted *discontinuous electricity*, that is, electrons, for Maxwell's electricity-continuum. A num-

ber of beautiful quantitative agreements were hereby gained, but the theory was not yet able to explain the relativity of optical phenomena.

(3) The alteration now required is that of substituting a *discontinuous emission in space* for the assumptions of continuous vibrations in the ether.

Thus the adjustment appears as a straightforward step in the progress of physics. It seems all the more natural as possibilities, hitherto unavailable, present themselves and enable us to gain the necessary coherence with regard to knowledge of the discontinuities in the emission of light—discontinuities which we have been forced to accept although they disagree with the Maxwell-Lorentz theory.

The problem thus reduces essentially to a single question. Which is more practical for human science, to procure an improved, *truer* theory of light, or to remodel all the fundamental conceptions of physics to which

we have grown accustomed, merely to be able to declare that the theory of light now prevalent is *good enough as it is.*

Everyone must judge for himself; but perhaps some may have become not wholly disinclined to accept in this case an observation made by Poincaré in a similar connection: "It is certain that we should find it more suitable not to alter our habits of thought." [1] For his part, as already stated, the present writer is of the opinion that we must accept light as being an emission in space; that is, we must admit that the velocity of light is not independent of the motion of its source.

[1] H. Poincaré, *La science et l'hypothèse.* "Il est certain que nous trouverions plus commode de ne pas changer nos habitudes."

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